the galls, and refers (by name simply) to the gall-forming animals; he arranges the galls according to the families of plants affected; he supplies more than a thousand serviceable illustrations, a statement of the geographical distribution of each gall, and the indispensable bibliographical references. The second volume is in the press; the first volume deals with the galls of cryptogams, gymnosperms, monocotyledons, and the dicotyledons from Ranunculaceæ to Rosaceæ. The work will be a great boon to entomologists, botanists, foresters, and agriculturists. We hope that the author will not write *finis* to his *magnum opus* without discussing, as he is so competent to do, the fascinating biological problems which are raised by the study of galls, crowning his work of description with an essay of interpretation.

Practical Coastal Navigation, including Simple Methods of finding Latitude, Longitude, and Deviation of Compass. By Comte de Miremont. Pp. 88. (London: J. D. Potter, 1908.) Price 4*s*.

IN this small volume Comte de Miremont has collected an enormous amount of useful information and what might be called tricks of the trade, which tell the young navigator everything that has been found useful in coastal navigation after years of experience.

The book is excellently arranged, and the explanations are simple. Besides the various chapters on actual coastal navigation, deviation and rule of the road, and weather forecasting in home waters, are most ably explained.

The book should find a place in every chart-house and navigation school. Comte de Miremont is to be highly congratulated on having produced such a useful aid to mariners, and to those wishing to become efficient in this particular art. H. C. LOCKYER.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Radiation of the Active Deposit from Radium through a Vacuum.

WHEN the radium emanation is transformed into radium A, the process is accompanied by the emission of α particles with a velocity of $1\cdot70 \times 10^9$ centimetres per second (Rutherford, *Phil. Mag.*, October, 1906). The portion of the atom from which the α particle has been emitted, which constitutes the radium A, must therefore be subjected to considerable shock and recoil in a direction opposite to that in which the α particle is projected. If we further consider that the mass of the α particle is $4(H=1)$, and that of the active deposit of the order 100, it follows that at the moment of its formation this product must be travelling with a velocity of the order 10^7 centimetres per second. In ordinary circumstances, when the emanation is mixed with air at atmospheric pressure, the radium A particle will possess only sufficient energy to permit it to travel a fraction of a millimetre before being stopped by collision with air molecules. On the other hand, at very low pressures, these particles should travel considerable distances without being stopped by the rarefied air, and come to rest on the enclosure containing the emanation. These particles should, in fact, constitute a type of very easily absorbed radiation. It has been the object of some experiments which we have recently performed to demonstrate directly the existence of this radiation.

The emanation from a fairly large quantity of radium was condensed at the bottom of a wide glass tube by immersing its end in liquid air. A brass plate, which just fitted into the glass tube, was suspended, in a high vacuum, a few centimetres above the condensed emanation so as to expose it to the bombardment of the active deposit particles being fired up the tube. After a suitable exposure the plate was removed, and its activity tested in the usual manner by a quadrant electrometer. The surface of the plate exposed to the emanation was always found to be highly radio-active.

Now this in itself would afford no evidence of the effect sought, for it is well known that when a large quantity of radium emanation is condensed in liquid air, the condensation is by no means complete, and there always exists in the vessel, above the condensed emanation, a considerable quantity of emanation in the gaseous state. A plate situated above the emanation as described above must therefore of necessity become radio-active on this account. But it was always found that the activity of the surface of the plate facing the emanation was greater than that of the opposite side, and it seems quite certain that this excess of activity is due to the direct radiation of the active deposit on to the plate. The ratio of the activity of the surface turned towards the emanation to that turned away from it has been found, under suitable conditions, to be as great as 50 to 1. The exact ratio obtained depends, of course, on a variety of experimental conditions, but in all circumstances the activity of the surface of the plate turned towards the emanation exceeded that of the reverse side. Moreover, by interposing a screen between the emanation and the plate, the excess activity collected on the surface of the plate turned towards the emanation could be completely obliterated. Experiments have been made at different pressures, and it has been found that the radiation is cut down to one-twelfth by traversing about 8 centimetres of air at a pressure of 1·15 millimetres of mercury. The same distance of air at 2 millimetres pressure is sufficient almost completely to stop the radiation.

These experiments give rise to a number of interesting questions which it is not yet possible to answer with any certainty. In the first place, it seems probable that when the emanation is condensed at the bottom of an evacuated tube, the attendant phenomena must be somewhat complicated, for when in radio-active equilibrium the emanation will be mixed with all its decomposition products. At every stage in the radio-active series at which α particles are expelled, some of the residual atoms should be fired up the tube. Although it is not yet possible to speak with certainty, it would seem that both radium A and radium B are projected up the tube on to the plate exposed to the radiation.

Another question of importance also arises as to whether the particles projected from the emanation are charged or not. Some experiments have already been made on this point with the object of deflecting the radiation by an electric field; but the difficulties are considerable, and no definite evidence has yet been obtained. We hope, however, that these difficulties will not prove insuperable.

S. RUSS
W. MAKOWER.

Physical Laboratory, The University, Manchester,
January 9.

The Isothermal Layer of the Atmosphere.

IT seems to me that in NATURE of January 7 (p. 281) Mr. Dines successfully defends his simple, compact, but extremely efficient apparatus from the suspicions that have been levelled at it. The tests of the instrument before and after use show that it truly records the temperatures and pressures to which it is reduced. Mr. Dines is therefore entitled to call for adequate discussion of the most marked outcome of the experiments—the fact that in nearly all cases the minimum reading of temperature is reached long before the maximum height in the ascent, and long after in the descent. To suggest that the thermometer or the barometer may be slightly out is really to evade the problem.

Taking, then, the readings as fairly accurate, do they prove the "isothermal layer"? What are the circumstances? To the best of my knowledge they are these:—the instrument is screened by a polished metallic cylinder open at top and bottom, the centre of which it occupies, and the draught of air produced by the up-rush and down-rush of the balloon is relied on to ensure that the thermograph, which is of light metal strip, shall take the

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temperature of the air with which it is in contact. It is hard to suggest a better arrangement for getting some notion of the low temperatures which the upper air appears to delight in producing and maintaining in the most trying circumstances; nevertheless, it is easy to show that the temperature of the instrument will differ from that of the air by a variable interval depending on the speed of the balloon, the density of the air, and the intensity of the radiation from earth, clouds, balloon, sun, air, and vapour.

Suppose I wish to take the temperature of the feed air below the fire-bars of a furnace. If the current of air were sufficient, and the screening of the thermometer almost perfect, I might get a close approximation, but if the draught relaxed in speed or density, or if the screening became less perfect, the thermometer would respond to the radiation by which it was surrounded and rise above the temperature of the air. A *ballon sonde* is such an apparatus. It is in a warm situation, but is surrounded by an intensely cold medium. It is a speck extremely close to a great warm planet, and bathed in his radiations and reflections—to say nothing of sun-rays, which sometimes complicate the problem. The screen is open to the earth below and to the balloon above, and the instrument, though screened itself, follows in the wake of the unscreened balloon and is fanned by the air that has passed over its heated surface.

In the ascent the thermometer reading falls briskly, and soon reaches a figure which may be 100° or more below what it would stand at if it were screened *from air currents* for a few minutes; but this gap between the temperature of the instrument and the temperature proper to its position cannot be extended indefinitely. The up-rush of the balloon attains a maximum velocity and declines, and the density of the air also rapidly diminishes. When the receipt by radiation equals the loss by conduction, the thermometer has reached its minimum, and enters the so-called "isothermal layer," the regularity of the occurrence of which on the traces is due to the similarity of pattern of balloon and outfit and of the other circumstances. I know that Mr. Dines contends that speed upwards or downwards can have no effect on the thermometer, but he takes no account of the circumstance that heat is constantly entering the instrument, and that it is solely the current of air that keeps down its temperature by removing the intruding heat.

It seems a pity that the following question stands barred:—Why *is* the material *Air* so cold where the material balloon and other instruments *would be* so warm?

R. F. Hughes.

16 Westmoreland Street, Marylebone, W., January 9.

If all balloon ascents had been made by day, I confess that I should be inclined to agree with Mr. Hughes and think that the recorded temperatures were due to radiation, but that idea is disposed of, to my mind, by the fact that the isothermal column of air shows just as plainly in ascents made after sunset as in those made in the day. At night the thermograph must receive some heat by radiation from the earth, and lose some by radiation into space, but both amounts must be infinitesimal in comparison with that which would be given to it by the sun. If, then, exposure to the sun does not seriously alter the temperature, and it does not do so even at the greatest height provided there is a moderate amount of vertical motion, the effect of the radiation after sunset must be utterly insignificant. That solar radiation in the ordinary conditions is not important is proved by the fact that if the balloon bursts, and therefore does not float, it is not possible to say from the trace alone if the ascent was by night or by day. There have been cases in which the balloon did not burst, and the temperature at the top reached the freezing point of water. If I asserted that the rate of ascent does not matter, I must plead guilty to a mistake, but I think I said "apart from radiation," and I still believe that radiation at night to and from the bright metal of the thermograph is so trifling that the rate of ascent is of no consequence. There is also the fact that the up-trace, where the motion is comparatively slow, is identical with the down-trace where the motion is rapid.

W. H. Dines.

An Electromagnetic Problem.

Since the solution of the problem put forward by Prof. Comstock in Nature of November 19, 1908, is apparently not obvious to everyone, will you permit me to point out that, so far as I can see, the difficulty arises, not from any peculiarity of the laws of electromagnetism, but from a simple misconception of the meaning of dynamical terms?

Prof. Comstock says that when an electrified sphere expands it loses electrostatic energy, but does not gain either kinetic energy (for the sphere has no mass) or magnetic energy (for the resultant field due to the motion of all parts of the sphere is everywhere zero). Now the energy of a conservative system, such as is considered, is measured by the amount of work which it can do on some external system in passing from its original to some defined final state; the amount of the work which can be done, and therefore the amount of the energy, will vary according to the external system which is chosen, and the principle of the conservation of energy will be true only if the same external system is taken in measuring the amount of work that can be done at various times during the change.

When he states that the magnetic energy of the expanding sphere is zero, Prof. Comstock is taking as his external system, on which work is to be done, a system unconnected with and independent of the expanding sphere; but the electrostatic energy of the sphere with respect to such a system is quite unaltered by the expansion, if the system is either wholly within or wholly without the sphere throughout the expansion, and the change in the electrostatic energy which ensues, if any part of the system passes through the surface of the sphere during the expansion, is independent of the discreteness or continuity of the electrification on the sphere, and perfectly consistent with the conservation of energy. Adopting such an independent system as that on which work is to be done, there is no relevant change in either the electrostatic or the magnetic energy.

On the other hand, when he says that the sphere in expanding loses electrostatic energy, Prof. Comstock is taking as the system on which work is to be done part of the expanding sphere or some system connected rigidly therewith; but then it is clear that in estimating the magnetic energy no account must be taken of the magnetic field due to the motion of this part. Leaving out of account the magnetic field due to this part of the sphere, a simple calculation shows that the magnetic field due to the motion of the rest of the sphere relatively to this part is *not* zero everywhere, but that the value of $\mu H^2/8\pi$, integrated throughout the entire field, is equal to the value of the electrostatic energy with reference to this part lost in expansion.

Norman R. Campbell.

Trinity College, Cambridge, January 16.

Radium in the Earth.

In the discourse entitled "Radio-active Changes in the Earth," delivered at the Royal Institution by the Hon. R. J. Strutt, and printed in Nature of December 17, 1908, the lecturer advanced the opinion that the mineral beryl contained an hitherto unknown element from which the comparatively large quantity of helium present is generated.

This interesting and remarkable conclusion has induced me to direct attention to a statement which occurs in a paper entitled "The Heat of Formation of Glucinum Chloride," by J. H. Pollok (Chem. Soc. Trans., 1904, p. 603). Mr. Pollok prepared a large quantity of basic glucinum carbonate from 2000 grams of beryl, and during the preparation of this compound he detected the presence of another substance, the nature of which he was not able to ascertain. His statement is as follows:—"This precipitate consisted chiefly of iron, zinc, and nickel sulphides, but another substance appeared to be present; its amount was, however, too minute to admit of any satisfactory conclusion being drawn regarding it. This sulphide has also been observed by Kruss and Moraht."

Percy Edgerton.

The Laboratories, 61 Cornhill, London, E.C.,
December 31, 1908.

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